Journal
of the
British Interplanetary Society

Vol. 6, No. 5. June, 1947

INTERPLANETARY FLIGHT: IS THE ROCKET THE ONLY ANSWER?

By A. V. Cleaver

(A paper read to the British Interplanetary Society in London on 1st February, 1947)

I. Introduction

In attempting to write a paper dealing with the question of whether the rocket is the only possible means for achieving interplanetary flight, it is difficult to resist the temptation of opening with Dr. Joad's well-known words: "It depends on what you mean . . ."

It is surely obvious what should be meant by the full achievement of interplanetary flight: the construction of a machine which will carry a useful payload of men, instruments, and equipment to provide for their essential services, from this planet to another and return, with a sufficient reserve of power to undertake all necessary navigational and landing manoeuvres, and to cover all emergencies which may reasonably be expected to arise. However, it is worth while setting down even this statement, because if anything less than this is meant by interplanetary flight, then the rocket has a better chance of filling the bill. If, for example, we are content to shoot a few pounds of instruments at the moon, without providing for their return, or (in any strict sense) for their safe landing, then this can be achieved very much more easily indeed than the task laid down here as constituting real interplanetary flight, which (contrary to the optimistic view sometimes expressed) is in an altogether different category of difficulty from that of more immediate rocket applications. It is perhaps not quite so obvious what should be meant by "rocket." On this question I will endeavour to show two things, viz.:—

1. If we mean a projectile propelled by an exhaust stream produced by combustion, or any purely molecular chemical reaction, then such a rocket cannot provide a really practical answer at all to the interplanetary flight problem.
2. If, on the other hand, we mean a projectile propelled by an exhaust stream produced by some other means, then this almost certainly is the answer sought for.

II. Ballistics of Space Flight

The escape velocity from this planet is about 11 km./sec., but it has already been postulated that true interplanetary flight involves much more than mere

escape. One way of regarding the additional requirements of providing power for the return take-off, landings, manoeuvring, and other contingencies (including gravitational and air resistance losses during the ascents) is to consider them as equivalent to the need for a greater velocity at "all burnt" than the mere escape velocity. At no time during the actual flight would the ship really attain this higher velocity, but it would be capable of doing so if the motors were left on to thrust continuously until all the propellants were exhausted.

Thus, on a typical interplanetary voyage, a possible velocity at "all burnt" of 30 km./sec. might be demanded; the moon trip might be done for only about 25 km./sec., but other journeys would entail much higher velocities. These questions are dealt with at length in reference (1) and elsewhere; in the same source, the well-known mass ratio relation is derived, from considerations of the conservation of momentum between the ship and its exhaust:—

$$\frac{M_0}{M_f} = e^{\frac{v_f}{v_e}} \quad .. \quad .. \quad .. \quad .. \quad (1)$$

This simple expression immediately reveals the fundamentals controlling rocket performance; to a large extent, the parameters of mass ratio and exhaust velocity are interchangeable. Higher final velocities are given to the ship if we have either a higher exhaust velocity or a higher ratio of take-off mass to final mass at "all burnt."

There are, however, obvious limits to the mass ratio which is possible from structural engineering considerations even if the true payload is reduced to only a negligible fraction of the final mass; the propellant tanks and hull structure of the rocket cannot be built for no cost in weight. In the V.2 the mass ratio was 3·4, and could have been about 4·5 if extra propellants had been substituted for the warhead "payload." Dr. Malina recently told this Society that he believed a mass ratio of 5·0 should be practicable with dense propellants, if the percentage payload carried was very small. If we accept the latter value, of $\frac{M_0}{M_f} = 5{\cdot}0$, then for a final velocity of 30 km./sec. to be imparted to the ship, we need an exhaust velocity of no less than 18·6 km./sec. (from equation (1)). No exhaust velocity lower than this would make such a final velocity possible, unless we could build a ship of even higher mass ratio, and even with a mass ratio of 10 we should need 13 km./sec.

The above figures relate to a single step rocket; with multi-step designs, much lower exhaust velocities could be made to suffice. However, the structural complication and various practical difficulties obviously increase with a great number of steps, while the further benefits gained become smaller after the decision has once been taken to employ a relatively small number. It seems unlikely that it would ever pay to go to more than five steps.

With a five-step ship having 5 tons true payload, it would appear that an exhaust velocity of only about 5 km./sec. would suffice to impart an effective final velocity of 30 km./sec., providing that we were prepared to accept a total take-off mass of around 150,000 tons! All these figures are intended only to

give a broad general picture; their absolute values are extremely sensitive to the assumptions made about mass ratio, just as the overall picture is also extremely sensitive to the available exhaust velocity. For the example given here, individual mass ratios of 5 were taken for all the first four steps, and 3·5 for the small final step which carries all the payload. However, even if the mass ratios assumed should prove to be very pessimistic after all, it is still obvious that higher exhaust velocities than 5 km./sec. are needed. Without them, even the step rocket principle does not make true interplanetary flight practicable, and it is a sobering thought that present-day exhaust velocities are of the order of only 2 km./sec. The next requirement in our study is clearly to examine the question of the highest exhaust velocities which we can ever expect from chemical propellants.

III. Limitations of the Chemical Rocket

In the past most astronautical enthusiasts have discussed the subject of exhaust velocity using the concept of the "ideal" value given by:—

$$v_i = \sqrt{2E} \quad .. \quad .. \quad .. \quad .. \quad (2)$$

Such velocities are quite meaningless as regards their absolute values, although they are useful for placing different propellants in a rough order of merit. They assume that all the available energy is transformed, with 100 per cent. thermal efficiency, into jet kinetic energy. This, of course, is impossible in practice; defining thermal efficiency as:—

$$\eta_{th} = \left(\frac{v_e}{v_i}\right)^2 \quad .. \quad .. \quad .. \quad .. \quad (3)$$

we find that existing rocket motors such as V.2 achieve a value of 25 to 30 per cent.

In the future it may be possible to raise this to 50 per cent., but even this will be difficult with the better propellant combinations. Certainly there is no hope of achieving anything approaching 100 per cent., or even 70 or 80 per cent., as has sometimes been suggested, especially if the propellants used have a high calorific value. In the latter event, combustion temperatures will be higher and dissociation effects correspondingly more important, thereby making it particularly difficult to achieve a good thermal efficiency. Thus, actual exhaust velocities for good propellants fall much shorter of the corresponding ideal values than is the case with inferior propellants—the gain from changing to a more powerful fuel is less than a comparison of the v_i values would suggest.

The best chemical propellants ever suggested have ideal velocities less than 6·5 km./sec. (The hydrogen and oxygen mixture gives 5·2.) With a thermal efficiency as high as 50 per cent., this would imply the possibility of actual exhaust velocities around 4·5 km./sec., but in the preceding section it was seen that this value was insufficient for our purpose. For that matter, even 6·5 km./sec. would also be inadequate; nevertheless, time may profitably be devoted to a brief survey of the reasons for maintaining the somewhat pessimistic attitude here adopted. Quite apart from the suggested unlikelihood

of thermal efficiencies greater than 50 per cent., it will be seen that influences are operating which make very high actual exhaust velocities improbable, even if future chemists succeed in inventing propellant combinations with higher "ideal" performances than that already mentioned.

Let us consider what happens to unit mass of propellants between the tanks and the nozzle exit:—

1. The propellants have to be raised from the tank temperature to that at which the combustion reaction takes place (i.e. at which the chemical change of state takes place from propellants to products of combustion). A fairly large amount of energy has to be supplied to do this—also a further much smaller amount to raise the pressure of the propellants to the injection value.

2. Combustion will not be 100 per cent. complete, i.e. all the propellant mass will not undergo the intended chemical reaction. Some heat will also be lost by radiation from the motor, but in a regeneratively cooled design, the energy absorbed by the propellant acting as a coolant is not lost—it is returned into the chamber.

3. After deducting the above sources of energy absorption, the remainder of the amount at our disposal (per unit mass finally ejected) is available to raise the temperature of the products of combustion. To this statement, however, there is one important reservation; this concerns the phenomenon known as dissociation, which is a different thing from incomplete combustion. Some of the remaining available energy, in most practical cases, will not go towards raising the temperature of the gas mixture in the chamber; as the chemists say, it becomes: "bound in the dissociated state."

This means that it is devoted to breaking down relatively complex molecules into simpler ones. For example, one of the products of combustion will probably be CO_2; as its temperature rises above about 1,500° C. in the chamber, an appreciable fraction of the weight of CO_2 present will break down into CO and O_2, with absorption of energy. This process will occur as a result of molecular collisions, which become more frequent, and which also have the dissociating result more often, as the molecules become more agitated at higher temperatures. It is true that in some molecular collisions, some CO and O_2 molecules will re-associate to form CO_2 once more, with evolution of energy. The process is essentially a two-way one, which never settles down and is always proceeding in each direction simultaneously. However, the application of statistical theory tells us that, at any given temperature and pressure, if we started with a certain weight of CO_2 after combustion took place, then at any given instant a certain average weight of CO_2 will be left, together with certain weights of CO and of O_2, and consequently with a certain energy loss.

Of course, similar phenomena occur with other products of combustion; CO_2 is merely cited as an example, and calculations can be made for all cases.

From calculations on the above three points—using data on the specific heats, latent heats and dissociation characteristics of all the substances involved —we determine θ_0, the temperature in the chamber. Account must, of course, be taken of the variation of specific heats with temperature. From the same calculations, we can also determine the specific heat ratio (γ) and the mean

molecular weight (m) of the gas mixture; the values derived should be the average ones appropriate to the range of temperatures applying between chamber and nozzle exit.

The maximum chamber temperature is thus seen to be almost entirely a function of the propellants used and the mixture ratio in which they are used. (The chamber pressure will have a secondary effect on the result because of the influence which it exerts on the degree of dissociation occurring.) We now have a mass of gas at a high temperature, and proceed to expand it through a convergent - divergent nozzle. At the minimum cross-section of this nozzle (the throat) the gases attain sonic velocity for the local temperature which exists there; a consequence of this is that the throat area used determines the chamber pressure (P_0) for a given mass flow of a given gas mixture. The permissible value of chamber pressure is, of course, decided by structural considerations.

During the expansion from chamber to exit, two further sources of loss occur; these are:—

4. The gases cannot be expanded down to zero exit pressure at the nozzle outlet, because this would require an infinite exit area.

In practice, this area cannot be much greater than that corresponding to the external pressure at the lowest altitude at which operation is required (e.g. sea-level atmospheric pressure). If it is, the consequent over-expansion may result in poor nozzle efficiency due to shock waves forming in the jet.

The chamber pressure being fixed, we are therefore left with a finite expansion ratio, which means that all the heat energy in the gases cannot be extracted. The actual value of the expansion ratio will determine the limiting efficiency of the thermal cycle.

5. Finally, there will be a further inevitable (but not large) loss due to friction and turbulence in the flow through the nozzle.

By the application of Bernoulli's Theorem for compressible flow between the chamber and the nozzle exit, it is possible to derive the following expression for exit velocity:—

$$v_x = \sqrt{\frac{2\gamma}{\gamma-1} \cdot \frac{G\theta_0}{m}\left\{1-\left(\frac{P_x}{P_0}\right)^{\frac{\gamma-1}{\gamma}}\right\}} \times \sqrt{\eta_n} \quad .. \quad .. \quad (4)$$

The thrust is then given by:—

$$T = \left(\frac{dM}{dt}\right)v_x + (P_x - P_e)A_x \quad .. \quad .. \quad .. \quad (5a)$$

or

$$T = \left(\frac{dM}{dt}\right)v_e \quad .. \quad .. \quad .. \quad .. \quad .. \quad (5)$$

In (5) the symbol v_e denotes an effective exhaust velocity which is slightly greater than the actual value because of the small residual pressure thrust term: $(P_x - P_e)A_x$. The equation (5) is thus simply a more convenient form to use in conjunction with such expressions as (1); the average value of the effective exhaust velocity, between sea level and outer space, will usually be about 10 per cent. higher than the actual value.

The foregoing is only a very condensed and inadequate survey of a highly technical subject, which is fully covered by, e.g., references (1) to (4); readers are referred to these for further information.

From equation (4) it is possible to make a more accurate estimate of the maximum exhaust velocity ever likely to result from a chemical combustion process. The argument may be developed as follows:—

(*a*) The variable γ, because of the way it occurs twice in the expression, does not very greatly influence the result; an average value is 1·2.

(*b*) Peak momentary pressures in the cylinders of internal-combustion piston engines have rarely, if ever, exceeded 100 sea-level standard atmospheres; usually they are very much less. If it is assumed that the designed nozzle exit pressure is 0·5 atmospheres, then the expansion ratio = 0·5 : 100 = 1 : 200. (Still better values for this variable would give only small further performance increases, and that only at the expense of a heavier motor and/or excessive nozzle sizes.)

(*c*) The main variable is thus seen to be $\left(\frac{\theta_0}{m}\right)$. The value of m is more or less fixed in magnitude by a knowledge of what gases can result from any possible combustion reaction. Thus, the combustion of hydrogen in oxygen (or ozone) gives the lowest possible value, which, after allowing for dissociation, is about 10·0. Most reactions, of course, will give a much higher value, and this is particularly true of some of the future possible higher-energy propellant combinations. Nevertheless, since a low value of m is favourable, the above figure of 10·0 will be assumed.

This leaves only the combustion temperature (θ_0), and it should be appreciated that practical rocket designers in the past, so far from seeking new fuels giving higher values of this parameter, have on the contrary sought to limit it by using even present-day fuels of relatively low energy content at mixture ratios either very rich or very weak, and/or by the addition of diluents, such as water, in those fuels. For example, on V.2 the fuel was not pure alcohol, but a water/alcohol mixture, so as to limit the temperature to less than 3,000° K.

It is also significant to note that peak momentary temperatures in the cylinders of internal-combustion piston engines never exceed 3,000° K. and are usually considerably lower than this; for other forms of heat engine, values are much less. Electric arc or induction furnaces may operate at 3,500° K., but even carbon melts at about 3,750° K., and tungsten at 3,600° K. (For most steels, of course, the melting point is not much more than 1,500° K.)

Taking all these factors into consideration, it will probably be agreed that considerable optimism is involved in the generous assumption of a permissible combustion chamber temperature of 4,500° K.

When all the above favourable values for the variables involved are substituted in equations (4) and (5), it is found that the maximum probable value of effective exhaust velocity (average between sea level and outer space) is only about 5·5 km./sec. Furthermore, a value nearer to 3·5 km./sec. is much more probable as a practical limit.

It should be realised that these values have been arrived at without any reference to the calorific values of any theoretical propellant combinations. They represent practical limits, whatever may be achieved in the latter connection in the future. As we have already seen, they are not enough for the achievement of true interplanetary flight, although they open up an impressive vista of possible rocket achievements falling short of that high ambition.

Before leaving this subject one special form of chemical rocket deserves mention, since it figures in most works on astronautics as a tantalising possibility. This is the use of mon-atomic hydrogen as a rocket mono-propellant, first suggested by Prof. Langmuir.[3] The source of energy would be the heat released by the re-association of hydrogen in its mon-atomic form into normal molecular hydrogen (i.e. $H + H \rightarrow H_2$). Thus, the process is still a chemical one, although it has nothing to do with combustion—no oxidiser is involved.

It must be realised that the energy input required to produce mon-atomic hydrogen in the first place is exactly the same as that released in the subsequent reconversion in the nozzle. The application of atomic hydrogen in welding will be familiar to many; molecular hydrogen is discharged through an electric arc, thereby absorbing energy which dissociates it into a jet of mon-atomic hydrogen. On re-association, this produces a very hot flame (about 3,500° K.).

Obviously, this input energy has to be supplied, so the system could find practical application in rockets only if one of the following two alternatives proved feasible:—

(*a*) If it were possible to store mon-atomic hydrogen in liquid form in the tanks, then the energy input could be supplied by ground installations before take off. The liquid mon-atomic hydrogen would then be injected into a reaction chamber comparable to the normal combustion chamber. Here it would re-associate into molecular hydrogen, which would be expanded through a nozzle in the usual way.

However, mon-atomic hydrogen is extremely unstable, and it is virtually impossible that it could ever be stored.

(*b*) That being so, it would be necessary to carry liquid hydrogen in molecular form in the tanks and provide electrical apparatus to convert it into the mon-atomic condition immediately before it was injected into the reaction chamber. Clearly this would be absurd if the electrical power had to come from accumulators, or generators run by prime movers which themselves consumed fuel. The scheme could be workable only in the most unlikely event that electrical power could be generated, for no expenditure of fuel and relatively little in fixed weight of apparatus, e.g. from the radiation energy available in outer space once the atmosphere had been traversed. (Such proposals have been made in other connections, and are discussed in the next section, but they are of an extremely visionary nature.) The obvious snag is the requirement for "relatively little cost in fixed weight of apparatus," which is impossible in any scheme involving intermediate machinery. Even if this were operated on atomic power, which would satisfy the condition of negligible fuel consumption, the fixed weight of turbo-generators and pile would

still be very large. Although only auxiliary machinery, they would still have to handle the same enormous power ratings as that of the final propulsive jet itself.

The theoretical attraction of the mon-atomic hydrogen proposal is, of course, the enormous energy released, corresponding to an ideal exhaust velocity of no less than 20 km./sec. Because, again, of temperature considerations, it is unlikely that it would ever be possible to use a complete reaction in which all the hydrogen passing through the chamber was initially mon-atomic. However, even if an incomplete reaction could be employed, limiting the temperature to a practical value, the system would still have immense theoretical attractions; the molecular weight of the gas efflux would be only of the order of 2, implying very high exhaust velocities for a given working temperature. Certainly the velocities attained would be far beyond anything possible with any other chemical process so far suggested, and relations such as (1) indicate how rapidly the ballistic performance improves with exhaust velocity. Hence, the idea has an inevitable fascination but the practical difficulties appear insuperable.

IV. Alternatives to the Chemical Rocket

The B.I.S. was founded in 1933; the R.Ae.S. in 1866. During the first fourteen years of B.I.S. existence, its members have continually met to debate the possibilities of an achievement which, for them, has always belonged to the unrealised future. This was also true of the R.Ae.S. during the first fourteen years of their history—indeed, for nearly the first forty. Perhaps we shall not have to wait so long; the tempo of events is quicker in this century. On the other hand, our wished-for achievement is much more difficult of performance.

The unattained but much sought after aim of the early R.Ae.S. was, of course, controlled flight by man within the atmosphere. They used to debate (with great heat at times) the relative chances of achieving this object with various types of then hypothetical flying machines. Nearly all their alternative suggestions have since been successfully developed—the airship, the aeroplane, the helicopter and even (on model scale) the ornithopter. Since we have come to the reluctant conclusion that the chemical rocket cannot fulfil all our requirements, what alternatives can we consider, comparable to those competitive devices which exercised the imagination of the early R.Ae.S. members? (Readers may regard much of what follows as being merely of possible historical interest, or light relief, or both.)

Apart from several very early suggestions, usually arising from ignorance of the fact that the earth's atmosphere does not extend indefinitely into space, there have been many more recent ones. For example, there have been several fallacious schemes for mechanical levitation devices.

The proposal of L. Theodore is shown in Fig. 1 and described in reference (5); about that date the correspondence columns of that periodical were full of discussion on similar schemes. The diagram indicates the inertia force (P) which the inventor neglected; this arises from the fact that the angular velocity of the oscillating mass (M) cannot, of course, be constant all the way from A to C.

With the mechanism shown, it varies all the way, but in any design it must change sign at A and C. Around the mean position B there can be a net lift on the device due to the vartical upward component of the centrifugal force (F), but nearer to position A or C the similar downward component of force (P) predominates. Thus, the device would undergo fluctuations of lift and down load corresponding to the net resultant of these two forces, but over the complete cycle A B C B A the total upward impulse (Lift × Time of cycle) would of course be zero.

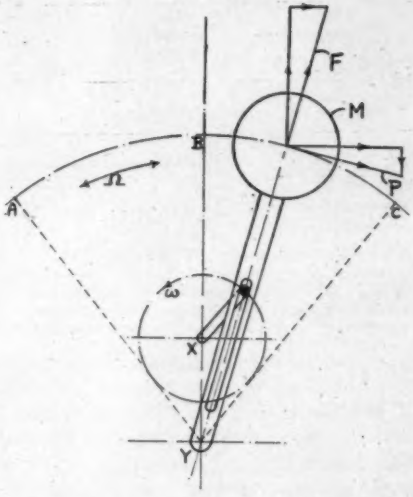

FIG. 1. Fallacious mechanical levitator of L. Theodore. The crank, with constant angular velocity ω about X, imparts a variable angular velocity Ω, about Y, to the mass M.

The same is true of the even more obviously fallacious proposal of H. W. Bull, an American inventor, who also made some early rocket experiments. His idea is shown in Fig. 2 and described in reference (6); at any instant there may be a positive upward force (f_1–f_2), but the net impulse over the whole period taken to bring both masses to rest back at the central position (O) is zero.

Apart from the fundamental fallacies in such schemes, it is evident that anyone submitting such ideas for space-ship propulsion can have no conception of the power expenditures involved. The thrust horse-power even of V.2 at "all burnt" was over 600,000; this quantity is, of course, the product of thrust and velocity of flight in appropriate units, so for a 100-ton projectile accelerating at 2 g. effective, the figure would be about fifty million horsepower by the time escape velocity was attained! These figures illustrate the absurdity of even searching for a solution which involves the use of rotating or oscillating machinery in a primary role; they also serve to emphasise the inescapable and

entirely fundamental difficulty of the task which we are attempting, by comparison with any past engineering achievement.

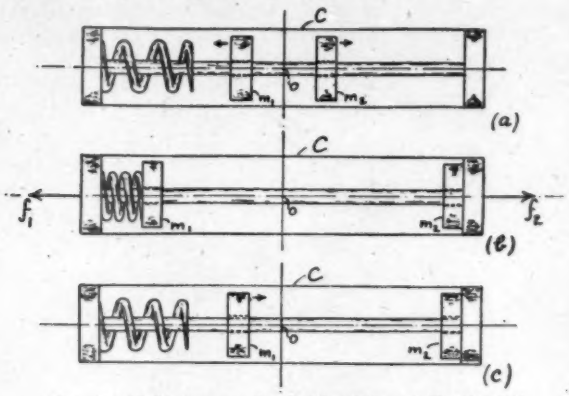

FIG. 2. Fallacious mechanical levitator of H. W. Bull.
(*a*) The masses m_1 and m_2 are flung apart by some means.
(*b*) m_1 is gradually arrested by impulse, m_2 suddenly by impact.
(*c*) m_1, however, is flung back, and needs stopping once again at O; this produces a further force (in same direction as f_2) on the cylinder C.

All students of astronautics are familiar with the many early proposals for using projectiles fired from guns to make interplanetary voyages. Several of the works of Jules Verne and H. G. Wells afford the best-known examples, but the idea was also employed (in conjunction with rocket propulsion) in one of the scientific essays of Professor J. B. S. Haldane.[7] No fundamental fallacy is involved in such proposals, but of course they are nevertheless quite impractical. The difficulty of the return trip is obvious, and if the acceleration used were limited to a value which the crew could withstand, the barrel length required would be impossibly large, even for such radical types of gun construction as that employed by the Germans for the Pas-de-Calais weapon intended to shell London. (This used successive propellant charges along an immense "drain-pipe" barrel.) The air resistance inside the barrel would have to be minimised by maintaining near-vacuo ahead of the projectile, and in any case the permissible muzzle velocity would be determined by the air density at exit.

Haldane appreciated all these points and endeavoured to disguise them in an atmosphere of plausible explanation. Verne apparently did not, and Wells just does not seem to have bothered; when he used the artifice of a "Space Gun" in his film romance "Things to Come," a shocked contributor to the then current B.I.S. *Journal* wrote a very disapproving review.[8] H. G. Wells then found a defender in Peter van Dresser of the American Rocket Society, who wrote in a letter to the B.I.S.:—

> "It is not unreasonable that an advanced science of the sort Mr. Wells postulates would make possible the construction of an electric gun, whose projectile was moved by the action of some sort of concentrated electrostatic

field acting on each individual molecule of the projectile and its contents, much as the force of gravity acts on the atoms of a falling body. In this way the effect of the high acceleration within the gun barrel would be nullified."

Van Dresser made it clear, however, that even he regarded this defence of a great man as somewhat tortuous and strained. For some reason Wells, who was usually both skilful and accurate in his scientific anticipations, seemed less particular than usual when he wrote about interplanetary flight. He was obviously more interested in the end than the means. If we knew enough of field theory to do as van Dresser suggested, then we could probably nullify gravity itself, without recourse to a gun.

In another of his stories,[9] Wells used this artifice, and then produced not an impracticability but a complete fallacy, which is wittily and scathingly disposed of by Ley in his recent book.[10] Of course, if we had Wells' mythical substance called "Cavorite," which was inherently not subject to gravity, then we could cheat all the laws of nature and achieve perpetual motion any time we chose. No expenditure of energy would be necessary; all we would need would be a Cavorite "lift" with controllable shutters to turn the effect on and off at will—weights could thus be raised to any desired height in the earth's gravitational field and released to do useful work by falling!

On the subject of gravitation some have been over-impressed by the well-known phenomenon of suspending objects made of some para-magnetic substance (such as brass or aluminium) over the poles of an alternating-current electromagnet. This is merely an eddy current effect, and other parlour tricks of levitation can also be produced by apparatus which balances magnetic fields against the earth's gravitational field.

At least one inventor, W. D. Verschoyle, was misled by electrostatic effects into the quite sincere belief that he could control the weight of objects. His anti-gravity experiments were quite widely reported in the Press about ten years ago (e.g. see reference (11), where the representatives of a technical magazine reported having seen the Verschoyle apparatus work successfully). Following Mr. Verschoyle's death in 1944, I secured the loan of his notebooks and arranged a repetition of his experiments. His apparatus, shown in Fig. 3, was extremely simple. It was claimed that, on making the circuit by closing the switch S_1, the so-called "radiator" (R) would experience a loss of weight and could even be made to have a negative weight. The radiator could have almost any form, although certain complex shapes were stated to have been found superior in Mr. Verschoyle's work, which extended over many years.

As fully anticipated, however, it was soon found that the "lift" experienced by (R) was merely the result of a combination of normal electrostatic repulsion and the reaction from charged particles streaming away downwards (i.e. "electric wind"). The force, in fact, could of course be made to operate either upwards or downwards; it was not found possible to obtain forces as large as Mr. Verschoyle claimed, and several of his conclusions regarding the variables which he thought to be important (such as the frequency introduced by the interrupter switch S_2) must have resulted from experimental error in using crude apparatus.

Verschoyle's beliefs were quite genuine, though mistaken; it would have been unscientific not to investigate his claims, since it was not utterly impossible that he might have discovered some fundamentally new effect. It was, of course, highly improbable; the day of great discoveries by private experimenters working with cheap and simple apparatus has probably gone for ever in the field of physics. However, there are no grounds known to the present writer for saying that means for controlling gravity will never be discovered. Equally, there are no grounds for suggesting that we have any notion whatever of how to set about the job, though future progress in physics many provide one.

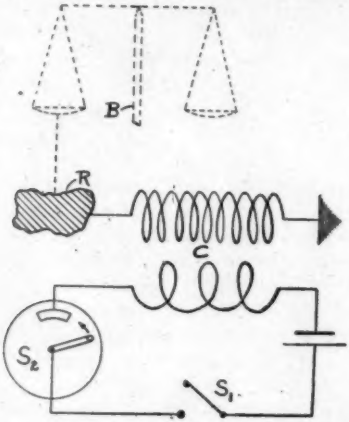

FIG. 3. Apparatus used by W. D. Verschoyle.
R = "Radiator," suspended from balance B. C = Induction coil.
S_1 = Switch. S_2 = Mechanically driven interrupter switch.

Authors of "science-fiction" have no such difficulties in the matter; they begin by passing on to their readers one of the current phrases used by popularisers of modern physics, such as: "Gravitation is the result of a curve in Space, caused by the presence of Matter." (Most such phrases are the result of an attempt to give a visual explanation of phenomena which really cannot be visualised accurately at all.) The next step is to explain that matter and energy are mutually convertible—as everyone knows since Hiroshima—and from this it is an easy process to pass to the "invention" of a plausible device which produces a "space-warp" in the opposite sense to the normal one, by means of the expenditure of energy in some form. Bodies in the region of this artificial "space-warp" therefore acquire a negative weight—what could be simpler?

Such a procedure may be technically dubious, but at least there need be no basic fallacy if the expenditure of energy is admitted in the "gravity nullifying" process.

Some day we may conceivably learn how to do this, but for the present it remains true, as the French physicist Borel[12] once wrote: "There was, however, something rather strange in this phenomenon of gravitation, something that

distinguished it from other physical phenomena. This was its utter immutability and its absolute independence of all external actions."

"Light is arrested by opaque bodies, deviated by prisms and lenses; electrical and magnetic actions are modified by the neighbourhood of certain bodies; gravitation alone remains always the same, and we have no means of enabling us to either increase or diminish it. Gravitation is indifferent to all physical circumstances, and is not affected by the chemical nature of bodies."

The difficulty of an experimental approach is obvious from the foregoing; it is interesting to speculate whether anything may develop in this respect from the future possibility of performing experiments in a ship under lengthy free fall conditions in space, or in positions of gravitational null points between two or more planetary bodies. In spite of the difficulties, many scientists, some with great names, have devoted themselves to experimental studies of gravity, all with completely negative results, although several were deluded for a time into thinking otherwise.

Faraday spent many years on the problem, working in the Waterloo Shot Tower on the South Thames Embankment, trying to establish a relation between gravitational and electrostatic or magnetic fields. The issues of *Science Abstracts* between 1919 and about 1925 contain many references to other investigations on this subject by many other scientists—including the Majorana brothers, Brush and Crémien—while some much earlier experiments by Austin and Thuring were made in 1897. The Majorana experiments, in particular, seem to have attracted much attention about twenty years ago, but their results were eventually shown to be spurious.

Gravitation, and all other physical phenomena, may be the inevitable consequence of the true geometry of the universe, which seems to be what modern physics has to tell us, but we can modify the effects of all phenomena other than gravity, as Borel said. More recently, Professor Max Born has suggested[13] that perhaps gravitational forces are a residue, due to incomplete compensation of electrostatic and electromagnetic forces inside the atom; he considers, however, that further speculation at the present time is premature.

Another great scientist of the last century who for a time thought he had discovered something about the physical nature of gravity was Sir William Crookes. Between 1872 and 1876 quite an extensive controversy raged in the scientific world about these experiments, and, oddly enough, also involved in these discussions was another question of possible astronautical interest, namely radiation pressure.

It is now well known that radiation can exert a pressure, the example of the effect most usually cited being the fact that the tails of comets, composed of very tenuous gaseous material, are pushed away from the sun when they approach relatively close to it. The suggestion has often been made that the principle might be used to provide an auxiliary means of propulsion for space ships, once they had been accelerated beyond the radiation shielding atmosphere by, for example, rocket drive. Thus Haldane, in the essay previously quoted,[7] suggests that voyages might be made at high velocity in outer space by methods rather analogous to those employed in sailing ships. The space

ship would have large reflecting surfaces, unfurled in space, on which the radiation pressure would operate.

More recently, Arnott made the same suggestion in a lecture to the B.I.S.[14]; he envisaged the possibility of using some atomic reaction, such as that which occurs in the explosion of an atomic bomb, to provide a strong artificial source of radiation pressure behind the ship.

The Italian, Gussalli, has also submitted a bizarre proposal[15] for the use of radiation pressure, with means to augment the thrust obtained. He envisages the projection of huge filaments of tenuous matter, similar to the classic case of the comet's tail, along the path of the space ship and behind it. These filaments would extend for a considerable distance, and could be sprayed from conventional rockets which might be exploded along the desired track. All the particles in such a filament would be acted upon by solar radiation, and because of their small mass would be accelerated to high velocities. The main space-ship itself, after climbing up through the atmosphere under rocket propulsion, would have spread its "sails" ready for these particles to impinge upon them, and produce a greater thrust than that of the radiation pressure by itself. In effect, the ship would be sailing along on a sort of artificially produced cosmic wind. Even if this proposal contains no basic fallacy, it is obviously open to many practical objections; the timing of the various operations, the economic aspect of the whole scheme, the structure of the "sails" to withstand the corpuscular bombardment, and the unlikelihood of its proving possible to develope adequate thrusts—all these are points on which the proposal can almost certainly be shown to be quite impractical.

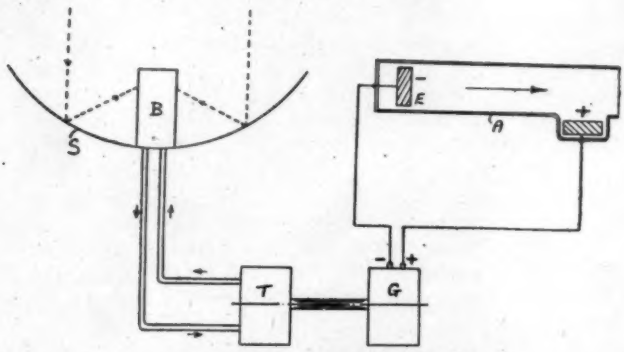

FIG. 4. Oberth's "electric rocket."
S = Mirror B = Boiler. T = Turbine. G = Electrical generator.
A = Electronic accelerating tube, with cathode E, producing propulsive jet.

Other and quite different proposals for the indirect use of the radiant energy existing in outer space have been made by Ulinski and Oberth. In the final chapter of his classic work,[1] Oberth discusses such schemes in considerable detail under the sub-title of "The Electric Space Ship." He concludes this rarely quoted section of his lengthy book—and the book as a whole—with the

somewhat naive statement that something along these lines, something better than the chemical rocket, will be needed to achieve true interplanetary flight, but whether this particular scheme will work he does not know.

Briefly, the electric rocket proposals of Oberth, and also of Ulinski (whose ideas were also described in one of the pre-war V.F.R. Journals), were along the general lines indicated in Fig. 4. The solar radiation was collected by huge surfaces (S) and the heat energy so obtained was transferred through the medium of boilers or heat exchangers (B) to a working fluid. This fluid operated turbines (T) on a closed cycle, and the turbines drove electrical generators) (G. Oberth suggested that these generators, to produce very high voltages, should be electrostatic machines rather than dynamos, as these would be lighter.

The electrical output of the generators was to be used to maintain a huge potential difference between the ends of an electronic accelerating tube (A). It was Oberth's idea that one of the electrodes (E) in this tube should be expendable, so that a stream of ionised particles would pour away from it and constitute a propulsive jet. Alternatively, of course, the applied voltage could be used to accelerate in a similar manner a stream of gaseous or dust particles fed into the tube. Yet again, if it were really possible to generate such large quantities of electrical power from solar radiation, it might theoretically be possible to use this in conjunction with mon-atomic hydrogen as a propellant, as mentioned at the conclusion of Section III.

Oberth was apparently quite serious about his "electric space ship" proposal; he printed various preliminary calculations suggesting the possibility of extremely high exhaust velocities, to justify his optimism that this might at last provide a source of motive power for real interplanetary voyages. Nevertheless, one cannot feel that the idea is of more than historical interest; as mentioned earlier, the quantities of power which require to be expended do not accord with what we have any reason to expect from rotating machinery of more or less conventional design.

Furthermore, it seems extremely doubtful, to say the least, whether worthwhile stores of radiant energy are available for tapping even in outer space, as is required for the schemes of Oberth, Ulinski and Gussalli to begin to have any practical interest. If they did exist at most points in space from which we might want to propel a space ship, then those points would probably not be very healthy localities for a space ship to be. Alternatively, if the radiation intensity was merely feeble, then such vast mirrors or "safls" would be needed to use it that the schemes would be impracticable on that score. Huge surfaces of this sort were, of course, envisaged by the writers quoted, who usually maintained that, in outer space, light structures of this nature would become practicable. However, one cannot visualise such surfaces capable of retraction within the hull of a ship, and also the inertia loads on such structures of great span would probably be prohibitive, even for quite gentle manoeuvres of the whole assembly.

V. Atomic Rockets

The preceding section began by discussing various suggested alternatives to the rocket, some of them fallacious, and all of them either impractical or

highly conjectural, and it ended with a scheme which was itself a specialised form of rocket, though making no use of a chemical reaction. It now becomes apparent that the basic rocket principle is almost certainly the best hope after all; indeed, the *only* one definitely known to us to be workable. However, we have to find a source of energy superior to that of chemical reactions, in view of the power/weight limitations which they impose, and we then have to discover a means of applying this to rocket propulsion.

Most astronautical investigators would of course agree that such a solution is now in sight, even if the prospect is still a distant one—though the present rate of scientific development may make that condition very short lived. As has become usual in all human affairs in this decade of the twentieth century, the factor which gives rise to this changed outlook is the advent of atomic energy.

Discussion of the possibilities of an atomic drive for space ships is, of course, not new, since the realisation of the inadequacy of chemical propellants is not new either. Even in 1939, the B.I.S. Technical Committee under J. H. Edwards were speculating on a proposed scheme for the secondary or indirect use of atomic energy. Their hopes centred on something very like Oberth's "electric rocket," but the potential difference between the ends of the propulsion tube was to be maintained by radiation from material, such as cadmium, made artificially radio-active in a cyclotron, before take-off. The mass of the propulsive jet was to be provided by gaseous or fine dust particles, accelerated through the tube. Many similar speculations—most, if not all, probably quite unworkable—are to be found in the pages of any interplanetary society's journal (e.g. see (16)). Since the atomic bomb, however, the rate of increase of such writings has been comparable to that of a nuclear chain reaction itself; the new element in the situation is that *now* there really is some hope of an atomic drive, whereas before it was just a dream which everyone believed would solve all our problems—some day, probably.

Even now the hope is not so very far removed from "such stuff as dreams are made on"; how much can be said with any confidence at this very early stage? Ackeret[17] has made an excellent preliminary investigation of the theory of an atomic reaction drive, incidentally developing all the relativity corrections which are important if the atomic energy is used in a direct manner, but which have been neglected by less well-informed writers on the subject. From a study of the principles laid down in Ackeret's paper, however, it seems probable that it will not be employed in that way; various practical considerations support this view, which Ackeret shares.

Ducrocq[18] and others have assumed that atomic energy will be used in a direct manner, the propulsive jet consisting of a stream of sub-atomic particles moving at velocities so high as to be an appreciable fraction of the velocity of light. (Or, as was recently suggested by a facetious writer in the aeronautical Press, having—by analogy with Mach No.—a fairly high "Maxwell No."!) It seems likely, however, that fissile material will be too expensive to be used with such a prodigal disregard of the low return in thrust from a huge given release of nuclear energy. The exhaust from such a motor would also be a distinctly lethal proposition, and it is difficult to visualise how it would be

handled, what practical form the equivalent of the nozzle would take, and so on. If the particles discharged carried a consistent electric charge, then perhaps the equivalent of the nozzle walls would be some sort of electrical guide field, but then the ship itself would become charged, and ejection would cease. If the particles were uncharged (neutrons) then, of course, such guide fields would in any case be useless, and the extreme velocities under consideration would be associated with fantastic temperatures.

Looking into the even more distant future, it is conceivable that we may become interested in some direct application of atomic energy to reaction propulsion when we come to consider interstellar voyages; for these, on account of the time factor, we would want the ship itself to travel at a high fraction of the velocity of light, and hence our return in thrust horsepower for a given expenditure of atomic energy would become reasonable. For the more modest flight velocities associated with mere interplanetary flight, however, it seems likely that we shall content ourselves with an indirect application of the great new power source! This latter task will perhaps be easier, though still of extreme difficulty.

Using a somewhat rough and ready argument, Dr. Hooker recently[19] pointed out that only a very small thrust would be obtained for a very large liberation of atomic energy, if the latter were used in the direct manner so far considered; he concluded that "atomic energy appears useless for rocket propulsion." It seems very strange that its indirect use, in the manner considered by Ackeret[17] and our own L. R. Shepherd,[20] did not occur to Dr. Hooker, particularly so in view of his enthusiasm for the exactly comparable development as applied to aircraft gas turbines.

In the latter, as in projected industrial atomic energy installations, an atomic pile would be used to heat up air (or some other working fluid), which would then be expanded through the power turbines. The turbine working fluid would be receiving heat from an atomic source instead of from chemical combustion; the same process could conceivably be applied in the rocket. The reaction chamber of such a rocket would become simply a heat exchanger unit, with the source of heat being some atomic device, which for the moment may be thought of as a plutonium or uranium pile. A suitable working fluid would be pumped into the chamber or heat exchanger, at a high injection pressure to give an efficient thermal cycle; it would then be expanded through a propulsive nozzle in the normal way. Fig. 5 shows such an arrangement in very diagrammatic form.

The working fluid would have to fulfil certain practical and theoretical requirements. On the first score, for example, it would have to be capable of being carried in the tanks in liquid form, to be pumped into the chamber, where it would vaporise and become a high-temperature gas. On the second score, the indications of equation (4) still apply, and demand that in its gaseous form the fluid should have as low as possible a value of mean molecular weight; maximum thrust will then be obtained for a given working temperature.

The latter consideration obviously suggests hydrogen as the choice for a working fluid, with a value of m of about 2 (actually very slightly lower because

of dissociation). We would then have attained the same conditions as apply for the hypothetical mon-atomic hydrogen chemical rocket, but without recourse to the chemical reaction of doubtful practicality which that idea involved. It should be realised, however, that the low density of liquid hydrogen is a severe disadvantage because of the large tank volumes, and hence weights, which it entails. Thus, in practice, it might prove preferable to use some substance such as ammonia as the working fluid; liquid ammonia has a specific gravity of 0·68 (as compared with 0·07 for hydrogen), and at a high temperature in the heat exchanger it would dissociate into a gas complex of nitrogen and hydrogen having a low molecular weight.

An atomic rocket motor of the above admittedly very conjectural type would necessitate a rather different outlook on the phenomenon of dissociation. We should be no longer primarily interested in conserving our stocks of energy to the last few per cent.; presumably the atomic heat source could be run at any energy level we chose within reason (and certain obvious limitations), making up any energy losses involved, such as that due to dissociation reactions, radiation losses, etc. In the chemical rocket, we start off with a stock of energy, and estimate the temperature in the combustion chamber—even if, for the sake of theoretical argument, we knew how to handle even higher temperatures, we have not the means to produce them. In the atomic rocket, on the other hand, we have the energy available to ensure that the working temperature will be whatever we decide; the estimation involved is of the necessary energy to be liberated, not the temperature.

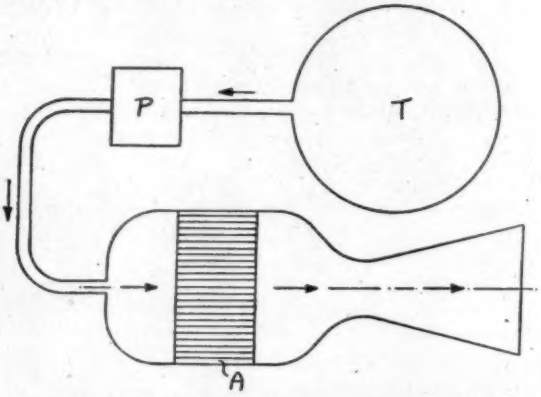

FIG. 5. Schematic of rocket using atomic energy indirectly.
T = Tank holding working fluid. P = Feed pump. A = Atomic heat source.

Thus, in practice, we would decide on a working temperature and ensure that it was attained, the value chosen being fixed by structural considerations. The system would show an advantage in exhaust velocity, as compared with a chemical rocket having the same chamber temperature, so long as the molecular weight of the exhaust gases of the atomic rocket was lower than for the chemical

one. The fundamental advantage of the scheme lies in the freedom of choice of working fluid which it offers; the selection can be made purely on a basis of molecular weight, without regard to whether any exothermic chemical process, of combustion or otherwise, will produce such an end gas. Hence, even hydrogen or helium could be used, although neither is the product of any combustion process, and although the only exothermic chemical reaction yielding the former is one which we do not know how to use.

Fig. 6 is a plot of exhaust velocity against chamber temperature for exhaust gases of molecular weight ranging between 2 and 8; it is calculated on the basis of equations (4) and (5), assuming an expansion ratio of 1 : 200, and $\gamma = 1{\cdot}2$, as for the earlier discussion of chemical propellant possibilities. Obviously, here at last we have hopes of obtaining those exhaust velocities, of the order of 10 km./sec., which would make real interplanetary voyaging practicable, and

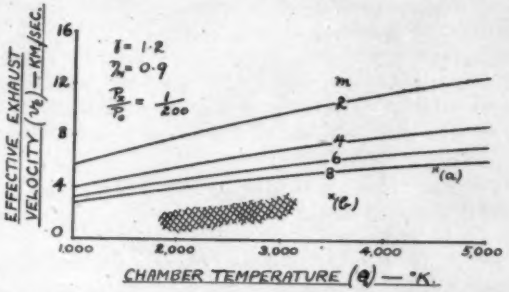

FIG. 6. Possible exhaust velocities from indirect use of atomic energy. m = molecular weight of working fluid; shaded area indicates regime of present chemical propellants, points (*a*) and (*b*) are guesses at possible ultimate performances with chemical propellants (for comparison).

still with maximum temperatures in our motors which we can conceivably learn to handle (i.e. < 4,000° K.). By way of comparison, the shaded area indicates the present-day region of operation for chemical rockets, while the spot points (*a*) and (*b*) correspond to the guesses made in Section III of the limits of chemical rocket performance—one highly optimistic, the other rather less so.

With an exhaust velocity of 10 km./sec., a two- or three-step ship with a take-off mass of only a few thousand tons might attain an equivalent "all-burnt" (we should now say "all-expelled") velocity of 30 km./sec., with a true payload of the order of 5 tons. The size even of such a ship as this would still be large, but it is at least within the practical range. However, the obvious fact must be stressed that we are still a very long way off from its realisation. The type of atomic rocket motor visualised here is only a hypothetical guess at what will probably develop; the following grave difficulties still bar the way:—

1. The weight of shielding against atomic radiation would probably be great, but might be kept within manageable limits by building the ship of a suitable shape. For example, the hull should be of high fineness ratio

(length/diameter), to take maximum advantage of natural shielding of the cabin by placing it in the nose, in the radiation shadow, while keeping the weight of shielding material to that minimum corresponding to a disc of relatively small diameter and the requisite thickness. Nevertheless, the weight of shielding may impose quite a large minimum possible size for atomic rockets.

2. The rather over-simplified arguments developed here have talked glibly of the operation of atomic heat generators at 3,000°–4,000° K. Certainly no conventional pile (if one may use that description of such a recent invention) could achieve this; the industrial piles which we shall see within this decade will operate at only a few hundred degrees. Uranium melts at less than 2,100° K., and one cannot visualise any form of pile remotely resembling present designs, with all their mechanism of movable nuclear reactors and control sleeves, running at the temperatures required for there to be any point in an atomic rocket.

   Perhaps these mechanisms could be replaced by a scheme of automatically controlling the energy level by thermal expansions of the lattice containing the reactors; perhaps new synthetic fissile materials will become available with higher melting points—perhaps it may prove possible to make some sort of gaseous pile.

However, the prize of success is so great (whether the world at large is yet prepared to admit it or not) that one feels sure solutions to these problems will ultimately be sought and found.

It is also conceivable that the enormous impetus recently given to research in atomic physics will, in the fullness of time, reveal ways of applying atomic energy to space flight at present quite unknown to us. Be that as it may, it seems almost certain that the answer to our problem lies in this direction.

VI. Conclusions

Summarising the results of this paper, the following conclusions seem justified:—

(A) The chemical rocket will probably be developed to give exhaust velocities of perhaps rather more than twice present-day values. Such progress will make possible very striking improvements in the performance of auxiliary rocket units for aircraft, guided missiles, and other rocket applications short of the ultimate aim of interplanetary flight.

   Quite extensive flights into outer space of either pilotless or piloted rockets will then become quite feasible, and will constitute a very necessary preliminary undertaking. Astronautical pioneers were quite justified, therefore, in advocating the development of the chemical rocket, even though many of them have long realised its ultimate inadequacy. It will be a vital step in astronautics, just as the airship was in aeronautics, even though it was eventually superseded by heavier than air machines.

   Nevertheless, it must clearly be realised that chemical rockets could undertake even the easier interplanetary voyages (such as to the moon

and back) only by quite fantastic disregard of the economy of the project, if at all. Payload would be only a microscopic fraction of the huge take-off mass required for ships of a many-step design.

(B) Some application of atomic energy to rocket propulsion, probably through the secondary use of an inert working fluid, seems more than likely to provide the ultimate solution to true interplanetary flight.

(C) As further possibilities, it is at least conceivable that some application of radiation energy or some new discovery concerning gravitational fields might enter the picture. The first of these seems very unlikely, however, while the latter is entirely conjectural; it would be unsound to regard either as being more than a remote possibility, compared with the probability of (B).

SYMBOLS

(* Alternative units are indicated in sequence.)

$\frac{M_0}{Mf}$ = Mass ratio.

= $\frac{\text{Take-off mass}}{\text{Final mass with all propellants consumed}}$.

v_f = Velocity of ship when all propellants have been consumed.

v_e = Effective exhaust velocity.
v_i = Ideal " " "
v_x = Actual " "
} * (m./sec., cm./sec., or ft./sec.)

γ = $\frac{c_p}{c_v}$ = Specific heat ratio.

G = Universal gas constant.
* (kg.-m./newton mol., ergs/grm. mol., or ft.-lb./slug mol., all per °K).

θ_0 = Combustion chamber temperature.
(° Kelvin = ° Centigrade Absolute.)

m = Mean molecular weight of gases.

P_x = Gas pressure at nozzle exit.
P_0 = Combustion chamber pressure.
P_e = External (atmospheric) pressure.
} * (kg./m.², dynes/cm.², or lb./ft.²)

T = Thrust. * (kg., dynes, or lb.)

$\left(\frac{dM}{dt}\right)$ = Mass flow of exhaust gases.
* (newtons/sec., grm./sec., or slugs/sec.)

A_x = Nozzle exit area. * (m.², cm.², or ft.²)

E = Energy content (calorific value) of propellants, referred to unit total mass of fuel plus oxidiser. *(kg.-m./newton, ergs/grm., or ft.-lb./slug.)

η_{th} = Thermal efficiency.

= $\frac{\text{Actual jet kinetic energy}}{\text{Input chemical energy of propellants}}$.

η_n = Nozzle efficiency.

$$= \frac{\text{Exhaust velocity with friction}}{\text{Exhaust velocity without friction}}.$$

N.B.

1 newton = the mass of a body weighing 9·81 kg. (at sea level).
1 slug = " " " " " " 32·2 lb. " " " .

References

(1) Oberth, H. *Wege Zur Raumschiffahrt.* Reprinted Edwards Bros. (U.S.A.).
(2) Sänger, E. "Recent Results in Rocket Flight Technique," NACA Tech. Memo. 1012 (U.S.A.).
(3) Bielkowicz, P. "Evolution of Energy in Jet and Rocket Propulsion," series of articles in *Aircraft Engineering*, March to June, and Dec., 1946, and Jan., 1947.
(4) Malina, F. J. "Characteristics of Rocket Motor Unit Based on Theory of Perfect Gases," *Journal of the Franklin Institute*, Oct., 1940 (U.S.A.).
(5) *Popular Aviation*, March, 1934 (U.S.A.).
(6) *Popular Science Monthly*, Jan., 1935 (U.S.A.).
(7) Haldane, J. B. S. *Possible Worlds*, essay "The Last Judgment." Pub. Chatto & Windus.
(8) Mayer, D. W. F. (Article by). *B.I.S. Journal*, Feb., 1937.
(9) Wells, H. G. *The First Men in the Moon.*
(10) Ley, W. *Rockets.* Pub. Viking Press (U.S.A.).
(11) *Flight.* Correspondence columns, July 25th, 1935.
(12) Borel, E. *Space and Time.* Pub. Blackie & Son.
(13) Born, M. *The Restless Universe.* Pub. Blackie & Son.
(14) Arnott, D. Reprint of lecture "A Survey of Atomic Power," *B.I.S. Journal*, June, 1946.
(15) Gussalli, L. *I Viaggi Interplanetari par mezzo delle Radiazioni Solari.* Pub. G. Vannini (Brescia, Italy).
(16) Journals of the American Rocket Society: April, 1940; Sept., 1945; June–Sept., 1946; etc.
(17) Ackeret, J. "Zur Theorie der Raketen," pub. in *Helvetica Physica Acta* (Basle, Switzerland), April, 1946, reprinted as translation in *B.I.S. Journal*, March, 1947.
(18) Ducrocq, A. "Le Moteur Nucléar," *L'Astronef*, June, 1946. (Paris, France.)
(19) Hooker, S. G. Reprint of lecture on "High Speed Flight," *Journal of the Royal Society of Arts*, March 29th, 1946.
(20) Shepherd, L. R. "The Problem of Interplanetary Propulsion," *B.I.S. Bulletin*, Nov., 1946.

REVIEWS

Geology Applied to Selenology

(By J. E. Spurr, Science Press Printing Co., Lancaster, Penn., U.S.A.)
Obtainable by special order from H. K. Lewis & Co., Ltd., 136, Gower Street, W.C.1.

Published under the above title is a very important book: important, not only for the vast amount of detailed information which it certainly contains, but also for the unique succession of interesting points raised.

Broadly speaking the book seeks to analyse in the light of terrestrial geology the surface features and structure of the Moon. More specifically it is divided into two parts, the first having already been published separately in 1944.

The first section, which is termed Vol. I, is called "The Imbrium Region of the Moon." Mr. Spurr's treatment of this region is exhaustive and extremely comprehensive. There is also an impressive background of references from ubiquitous sources, which is further strengthened by frequent cross-referencing.

The author opens his analysis by describing the characteristics of some of the main types of lunar crater, introducing geological terms and following with an application of the same treatment to general formations of the Imbrium Region. He identifies geologically such features as the subsidence of the plain itself, the appearance of the islands in the mare surface, the wrinkle ridges and the detailed phenomena characteristic of some of the main craters—ray formations; block ejectamenta; channelling down some of the crater slopes and similar features.

A dissertation on "rays" then follows, with particulars of experiments conducted by his son, Dr. Robert A. Spurr, comparing the distribution of fine powder by explosion both in air and *in vacuo*.

Such experiments have been carried out before by investigators with similar decisive results, namely, that the formation of a ray system is dependent upon a lack of atmosphere, the presence of which causes prolonged drifts of dust particles before reaching the ground and resulting in a more or less even distribution.

Then follows a classification of the depressions in the Moon's surface, starting with the largest of the mares and ending with the small crater pits and craterlets which he calls blowhole craters. The author develops a transitional sequence in which walled plains or ciques representing most of the older geological depressions are transitional from the mares. "Caldera craters," represented by Eratosthenes and Copernicus, follow the ciques, and the sequence ends with the aforementioned blowhole craters of which the smallest are known as craterpits and craterlets in astronomical terms.

From there on Vol. I analyses, compares and deduces. In the course of an investigation into the nature of the Moon's crust, rectilinear formations sometimes noted in the shape of craters are explained and chemical compositions discussed.

The formation sequence of the plain itself is suggested, moving from an igneous cycle to the formation of the fault system around its edges.

The craters conspicuous in the confines of the plain are minutely discussed, "patterns" developed, and interior craters and features relegated to places in these patterns. Both the unusual and characteristic craters are dealt with in the course of the rest of this volume.

Flow lines visible on the photographs are termed "wrinkle ridges" and are discussed in connection with the formation sequence of Mare Imbrium and its companion mares which border its faulted and mountainous limits. The domed formations which underlie some of the craters are explained as being domical uplifts and are of the same category as the preliminary stages of the formation of the ciques and mares, according to Mr. Spurr's reasoning. Certain domes which have no craters upon their surfaces take their place as abortive attempts of the gases underlying the fast-hardening magma to break out.

Moving away from Mare Imbrium, the "no-man's land" between this and the Mare Frigoris is analysed. It provides, among other things, a basis for building up a reasonable picture of the relative geological processes which went on between the two plains.

Further North still, the relation between the ancient lands of the North and Frigoris are studied in a like manner.

Of particular interest is the tracing of the origin of a series of "rays" to a source well beyond the limb of the Moon—yet another piece of the puzzle of what lies on the unseen half of the Moon. All the pieces up to now have supported the logical conclusion that the Moon is the same all the way round, so to speak. There may, of course, be slight differences in general surface texture, due to the fact that lunar eclipses only affect the sunward side of the Moon. In other words, the seen side of our satellite receives an extra helping of temperature changes which undoubtedly cause additional exfoliation.

The logical result is that the visible hemisphere of the Moon may be a little more superficially riven and powdered than the invisible one. Not a very sensational difference, but all that can be expected.

The North and South of Frigoris are compared thoroughly, followed by a summing up of the geological sequence of the whole region.

On the basis of past analyses it is now possible to compare the histories of the Imbrium and Frigoris regions. This Mr. Spurr does. He goes on to apply the same evidence to the formation of the different major groups of crater formations—the pre-mare and the mare.

Vol. I ends with an attempt to place certain of the craters in lines, which, the author states in support of other selenologists, lie on planes of weakness. Depth fissures, which the author draws in conjunction with the crater-chains, are explained as being deep-seated faults which, among other things, are related to a much larger system than the Imbrium fault system.

The final chapter is devoted to a comparison between the geology of the Earth and the Moon. So ends Vol. I.

Vol. II is an application of the same technique of analysis to the Moon's visible surface as a whole and although it is over twice as long as Vol. I, it does not deal as exhaustively with the whole visible hemisphere as with the Imbrium region. That would obviously take a small library full of volumes to cover the task. It does the next best thing in that, to quote Mr. Spurr's own words, ". . . the method has been to select and analyse such types and type areas as would, taken together, contribute to a general treatment, leaving most of the detail unmentioned, and to some extent unstudied."

It would be as impossible to review minutely this section of the book as it would be to deal with a dictionary. The wealth of detail is enormous. Suffice to say that having read Vol. I, the reader is better equipped to study the Moon's surface as a whole in the way it is dissected, classified, stratified, pattern sequenced and typed in Vol. II.

However, having read the book, several impressions remain, not all of which are favourable. The favourable points have already been mentioned, namely, the wealth of detail and exhaustive analyses backed by a deep knowledge of terrestrial geology. But there are several other facts which should be mentioned.

In the first place the author seems to be far too wrapped up in igneous geology, culminating in a very offhand Addendum to Vol. I, in which he states,

as an afterthought, and apparently just to humour a certain section of the astronomical public, that he thinks perhaps he had better mention that silly theory about meteoric formation of some of the crater formations. He admits that the usual belief is that Coon Butte in Arizona is the result of meteoric bombardment and that this instance is usually quoted in support of that theory. However, he says, "if meteoric bodies had been an important factor in geological history we should find record of them in smashed rock strata and meteoric detrius." This cannot be found, it is true.

He also says that we cannot expect the earth to have received less than the Moon in meteoric bombardment in the past; and that evidence of present meteoric erosion of both Earth and Moon is negligible. This is again true.

The author seems to forget, however, that one of his conclusions was that crater formation on the Moon must have taken place in its entirety at a very early period in its geological history. In addition to this he asserts that the Moon must have cooled much more quickly than the Earth and must have formed the crust which caused the peculiar geological characteristics of the Moon to become operative.

From these two conclusions we are forced to the view that if indeed the craters *had* meteoric origin due to a localised stream of special debris or some such cause, and that the Earth received its share at the same time, it is more than probable that the surface of the more massive Earth would show not a sign of the impacts for the simple reason that at that time the Moon had a thin crust, while the Earth would still be comparatively molten. Thus one of the author's main objections becomes void.

As to the fact of the presence of spacial debris at that time, it seems logical to assume that whatever the true origin of the solar system, it must have caused, as well as the formation of the planets, a mass of smaller condensed matter. That which lay in the paths of the planets would be swept up in the early stages of cooling, *possibly when the Earth was still molten and the Moon crusted*.

Or perhaps the Moon was captured by the Earth at some date after the formation of the craters. Whatever the real reason there still seems more room for speculation than Mr. Spurr allows.

It seems, too, that the author banks too much on the viscosity of the Moon magma in the formation of the craters and in the assumption that they retained their sharpness by instant cooling. Even taking into consideration the lack of atmosphere in the increased heat loss by the moon, it still seems a little doubtful that it would cool so quickly.

Again, in view of the fact that the author has put an igneous origin to all the craters, it seems likely that most would exhibit lava emission. Such is not the case. In fact, out of all the craters in the Imbrium and Frigoris regions only two or three such flows are noted.

Finally, it would be possible to apply much of the geological evidence gathered by the author to other regions.

The foregoing are the only legitimate criticisms that might be made, but they do not alter the fact that this book represents a major step forward in the

geological investigation of our satellite, and nothing quite like it has ever before been published. It presents reasons, theories and explanations in such a convincing and logical way that disagreement can only be effective if backed by equally high standard of logic and demonstration.

One thing is paramount, Mr. Spurr knows his geology !

M. W. WHOLEY.

Dawn of the Space Age

(By Harry Harper. Published by Sampson, Low, Marston & Co., Ltd., 43 Ludgate Hill, London, E.C.4. 142 pp., illus. Price 8s. 6d.)

In this book Mr. Harper has made use of his great experience in the art of writing non-technical books on technical subjects to produce what is undoubtedly the best book of this kind so far published in this country. It is, in effect, a summary of all the published work of the British Astronautical Movement and Mr. Harper has been content to stick to the material provided without drawing on his imagination for those irritating flights of fancy we have come to expect as the inevitable flowerings of journalistic art.

The book is divided into three sections, the first, historical; the second delineating the objectives of space flight; and the third being a complete description of a pre-war Lunar space-ship design.

It is not intended to be a book for the technician, but rather as the sort of book technicians can use as a means of explaining the subject to the uninitiated.

There are twelve illustrations, including drawings by Bruce Gaffron and an introduction by K. W. Gatland. The book has received the official approval of the Technical Advisory Committee of the B.I.S. as a non-technical work.

R. A. SMITH.

Jet Propulsion Motor Models—Instruction Manual

(Published by Aircraft Jet & Rocket Corporation, N.Y.; obtainable from P.P. Agency, 52, Halton House, 20-23, Holborn, E.C.1. Price 8s. 6d.)

This book is of American origin and is chiefly of interest to the person who has good workshop facilities at his disposal. It consists of about six pages of text and nine sheets of detailed drawings, showing how to fabricate two different types of jet motors. The text consists simply of fitting and erecting notes with a little amplification of detail and particulars of the method of starting up the completed motors.

The two types of motor described are (1) the resonance jet motor and (2) the ramjet or pure athodyd. Drawings from one to seven deal with the former, while the remaining two refer to the athodyd. The drawings are very clear, giving much detail in full scale, and also include suggested rig-ups for application to actual working models.

However, the work in making either one of these motors is beyond the scope of the average amateur and the fitting calls for a very high degree of skill. Further, if proper precautions are not taken when starting and running, very serious consequences might result. Another drawback to these motors is the fact that their inflexibility for adaption to provide power for any other purpose other than where direct thrust is required.

It should not be inferred from this that the would-be experimenter should turn down this interesting line of work, as it is only by development that such motors can reach a pitch of efficiency comparable with other prime movers. Some time ago news-reel pictures were shown of a contest held in America using flying models powered with motors of the resonance type. Unfortunately the film was of short duration and the shots were not close enough to show much detail. Several of the models took off and flew very well, emitting the characteristic exhaust note of the V1. Others in the competition failed to get away, and one unfortunate, after zig-zagging along, turned right over and promptly became a blazing mass. But this goes to prove that, providing these models are correctly made and adjusted, there is no reason why they should not function properly, though much patience is required before this state of affairs can be reached.

Probably the easier of the two models to make is the athodyd, but, unfortunately, this motor has the characteristic that it develops no thrust when stationary, and some method of launching a plane so powered would be required to give sufficient speed until the motor could function. Once the motor took over the speed of the plane would increase, this in turn building up a higher thrust, until a very high velocity was reached. At these high speeds the athodyd is the most efficient type of jet motor. Thrust-tables are given in the text of the book showing the characteristic values for performance.

The resonance motored plane should, if properly adjusted, take off itself, although the rate of acceleration would probably not be quite so high, nor also would be the final velocity. Furthermore, adjustments would have to be carried to fine limits, and this is somewhat difficult, especially with motors of the size shown on the drawings.

H. E. FIELDEN.

ABSTRACTS

Flight

The issue dated 20th March, 1947, contains a two-page article, with photographs and drawings, describing the Reaction Motors 6,000 C.4 rocket power plant of the Bell XS—1 supersonic research aircraft. The American motor is compared with the Walter design used in the German Me.163C interceptor fighter.

The information on the Reaction Motors unit was obtained from a recent article in *Aviation* by John Shesta, well known as an old A.R.S. member and now technical director of Reaction Motors, Inc. One wonders whether the weight of 210 lb. quoted for the 6,000 C.4 engine is inclusive of all auxiliaries, since it appears extremely low for even a rocket motor of 6,000 lb. thrust.

Aircraft Engineering

The January, 1947, issue contains the final article in the excellent series "Evolution of Energy in Jet and Rocket Propulsion," by P. Bielkowicz. It deals further with dissociation and other thermo-chemical problems of the rocket motor, concluding with the deviation of the well-known expressions for thrust and exhaust velocity in terms of operating conditions.

CORRESPONDENCE

Count von Braun

SIR,

I expect that many members will be interested in a short biographical sketch of Count Wernher von Braun, though I am afraid that the job I am undertaking in doing so is rather like attempting to draw the map of the moon from memory.

The fact is that I have known Count von Braun for a period of several years, although not very intimately. I also have to admit that I did not pay more attention to him then than to any other active member of the *Verein für Raumschiffahrt*, at any event not as much as I surely would have done had I been able to look into the future.

Let us see now what I can recall of the sequence of events. As a point of departure I will use the date of the World Premiere of the Fritz Lang film *Frau im Mond* which I remember well. It was in Berlin on the 15th of October, 1929. I am fairly certain that Fritz Lang had invited only two representatives of the VfR, Professor Hermann Oberth and myself. During the winter that followed, the winter from 1929 to 1930, Oberth went "home to Mediash" and the headquarters of the VfR were transferred from Breslau to Berlin, first factually and then legally. We used the office of Patent Attorney Wurm for headquarters, and in that office a meeting took place where Rudolf Nebel (Oberth's assistant during the rocket experimentation sponsored by Fritz Lang and the UFA Film Co.) became Acting Secretary of the Society. Two or three meetings later a (then) young engineer by the name of Klaus Riedel showed up. But von Braun was not yet in sight, at least not as far as society business was concerned.

Although I handled the membership lists at that time I cannot recall whether von Braun was on the list or not, either case is possible. But I do remember that he visited me in my home one day, presumably that was during the summer of 1929. Mostly because I have had many young visitors with the same request since then I can recall what he came to see me about. He told me that he had graduated from high school some time before that and was going to study physics and/or engineering. He was greatly impressed with the possibilities of liquid fuel rockets and had decided to devote his life to that problem. Question: "Which subjects should I study in order to qualify as a researcher in this field?" The question was not worded in this manner, but that was the meaning.

During the Spring following the Winter 1929-30, Prof. Oberth returned to Berlin and the experiments at the *Chemisch-Technische Reichsanstalt* near Berlin were made. During these experiments Oberth had three helpers, one who occupied himself mainly in driving around and getting tools and materials, Nebel, and two who settled down at the work bench: Riedel and von Braun. I know the date of the final experiment because of its official certificate; it was 23rd July, 1930, and I have a photograph taken a few days earlier which shows Oberth, Nebel, Riedel and von Braun. Wernher von Braun must have appeared

on the scene between March and May of that year. Just how this happened I do not know, but I suppose that he may have visited Oberth the way he had visited me a year earlier. I also remember for some reason that he was about 18 years then.

From then on I saw von Braun on and off at the rate of about once a month until he was swallowed up by German military secrecy. Physically he happened to be a perfect example of the type labelled "Aryan Nordic" by the Nazis during the years to come. He had bright blue eyes and light blond hair and one of my female relatives compared him to the famous photograph of Lord Douglas of Oscar Wilde fame. His manners were as perfect as rigid upbringing could make them. I remember that he spoke a rather good French. One day he came in while I was struggling with a Sarabande by Handel; after I had finished he sat down and played Beethoven's Moonlight Sonata from memory.

There was no doubt even then that he was brilliant. Riedel often asked von Braun to make calculations for him and to assist him in the proper dimensioning of injection nozzles, etc. I knew that von Braun was studying in Berlin then and working rather hard at it which prevented him from giving as much time to the VfR's experimental work as he otherwise might have done. He told me that he wanted to get his doctor's degree in engineering from the Swiss Federal Technical College in Zürich. Since he is now referred to as Dr. von Braun he undoubtedly got his degree, but I do not know whether he got it in Switzerland as planned.

Did we discuss politics? Hardly, our minds were always far out in space. But I remember a few chance remarks which might be condensed into saying that in von Braun's opinion (as of that time) the German Republic was no good and the Nazis ridiculous. That, of course, was simply the political platform of the *Deutsche Adels Gesellschaft* (Society of German Nobility) to which von Braun's father and possibly also Wernher von Braun belonged. The first name "Wernher," incidentally, is now very rare, but was frequent in noble families during the Middle Ages; it may have been in his family.

In Summer, 1932, President von Hindenburg appointed a new government headed by Franz von Papen (see Proceedings of the Court at Nuremberg) as Chancellor. In the cabinet nominated by Franz von Papen there was a Freiherr von Braun; he was the Minister of Agriculture. He also was, as we learned with considerable surprise, the father of our young engineer by the same name. Simultaneously Wernher von Braun became less accessible. I presume that the groundwork for his connection with the German Army was laid then. Simultaneously Nebel started a campaign to have him elected as a member of the Board of Directors. Some members of the Board objected. Yes, they said, of course it would be nice to have the son of a Reichsminister as a member of the Board. Yes, they added, we don't doubt his ability and his scientific usefulness after completion of his studies. BUT, gentlemen, but the young man is hardly twenty years of age. "I am not sure, gentlemen (Wurm speaking) whether this is a legal obstacle or not, but even if not we cannot have such young men around."

I can still see the coffee table at the *Raketenflugplatz* where this meeting took place and I can still visualise the mixture of gravity and excitement with which Wurm uttered the accusation. And I still remember Willy Ley rising and saying, "Gentlemen, if von Braun's age is the only obstacle you can think of, rest assured that that will be bettered every day all by itself."

Wernher von Braun was elected.

I have occasionally said that history might have been different if I had kept my mouth shut on that occasion, but I really do not think so. The further development of the rocket story did not rest with the VfR any more, but with the German army, and when von Braun resigned as a member of the Board of Directors of the VfR—only a few months later—the decision had probably been made.

That was about the end of our association, but I did run into von Braun accidentally once or twice later on. On one of these occasions he made the plaintive remarks I have quoted in my book.

Of the events that followed I have no direct knowledge; what I know is pieced together from released documents and newspaper interviews. Some four years after von Braun left the Vfr the German army founded the Peenemünde research institute. In August, 1943, it was smashed by the R.A.F., but von Braun as well as Oberth survived the heavy raid—Oberth, according to a statement he wrote after his capture, left Peenemünde soon after because of differences of opinion with von Braun. The latter fled when the Red Army approached and was captured by American troops in Bavaria after he had suffered an automobile accident.

Later he and a group of other German rocket experts and technicians were brought to the United States to assist in carrying out the so-called rocket sonde programme, undertaken by the U.S. Army in collaboration with all kinds of scientific institutions, using captured V-2 rockets. The status of these men, as announced by the Army, is that of "special employees" which term usually implies temporary employment. What their status is going to be in the future has not been announced; presumably no decision has been made.

WILLY LEY.

BOOKS on PURE AND
APPLIED SCIENCE ALWAYS AVAILABLE
Please state interests when writing.
H. K. LEWIS & Co. LTD. 136 GOWER STREET, LONDON, W.C.1.
Telephone: EUSton 4282 (5 lines).

PRINTED BY W. HEFFER & SONS LTD., CAMBRIDGE